

SCIENCE.

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ON THE MAGNITUDE OF THE SOLAR SYSTEM.*

NATURE may be studied in two widely different ways. On the one hand we may employ a powerful microscope which will render visible the minutest forms and limit our field of view to an infinitesimal frac-

*Part of the Address delivered before the American Association for the Advancement of Science at its Brooklyn meeting, August 16, 1894, by the retiring President, Professor Harkness, and reprinted with his permission.

tion of an inch situated within a foot of our own noses; or on the other hand, we may occupy some commanding position and from thence, aided by a telescope, we may obtain a comprehensive view of an extensive region. The first method is that of the specialist, the second is that of the philosopher, but both are necessary for an adequate understanding of nature. The one has brought us knowledge wherewith to defend ourselves against bacteria and microbes which are among the most deadly enemies of mankind, and the other has made us acquainted with the great laws of matter and force upon which rests the whole fabric of science. All nature is one, but for convenience of classification we have divided our knowledge into a number of sciences which we usually regard as quite distinct from each other. Along certain lines, or more properly, in certain regions, these sciences necessarily abut on each other, and just there lies the weakness of the specialist. He is like a wayfarer who always finds obstacles in crossing the boundaries between two countries, while to the traveler who gazes over them from a commanding eminence the case is quite different. If the boundary is an ocean shore there is no mistaking it; if a broad river or a chain of mountains it is still distinct; but if only a line of posts traced over hill and dale, then it becomes lost in the natural features of the landscape, and the essential unity of the

whole region is apparent. In that case the border land is wholly a human conception of which nature takes no cognizance, and so it is with the scientific border land to which I propose to invite your attention this evening.

To the popular mind there are no two sciences further apart than astronomy and geology. The one treats of the structure and mineral constitution of our earth, the causes of its physical features and its history, while the other treats of the celestial bodies, their magnitudes, motions, distances, periods of revolution, eclipses, order, and of the causes of their various phenomena. And yet many, perhaps I may even say most of the apparent motions of the heavenly bodies are merely reflections of the motions of the earth, and in studying them we are really studying it. Furthermore, precession, nutation and the phenomena of the tides depend largely upon the internal structure of the earth, and there astronomy and geology merge into each other. Nevertheless the methods of the two sciences are widely different, most astronomical problems being discussed quantitatively by means of rigid mathematical formulæ, while in the vast majority of cases the geological ones are discussed only qualitatively, each author contenting himself with a mere statement of what he thinks. With precise data the methods of astronomy lead to very exact results, for mathematics is a mill which grinds exceeding fine; but, after all, what comes out of a mill depends wholly upon what is put into it, and if the data are uncertain, as is the case in most cosmological problems, there is little to choose between the mathematics of the astronomer and the guesses of the geologist.

If we examine the addresses delivered by former presidents of this Association, and of the sister—perhaps it would be nearer the truth to say the parent—Association on the other side of the Atlantic, we shall find

that they have generally dealt either with the recent advances in some broad field of science, or else with the development of some special subject. This evening I propose to adopt the latter course, and I shall invite your attention to the present condition of our knowledge respecting the magnitude of the solar system, but in so doing it will be necessary to introduce some considerations derived from laboratory experiments upon the luminiferous ether, others derived from experiments upon ponderable matter, and still others relating both to the surface phenomena and to the internal structure of the earth, and thus we shall deal largely with the border land where astronomy, physics and geology merge into each other.

The relative distances of the various bodies which compose the solar system can be determined to a considerable degree of approximation with very crude instruments as soon as the true plan of the system becomes known, and that plan was taught by Pythagoras more than five hundred years before Christ. It must have been known to the Egyptians and Chaldeans still earlier, if Pythagoras really acquired his knowledge of astronomy from them as is affirmed by some of the ancient writers, but on that point there is no certainty. In public Pythagoras seemingly accepted the current belief of his time, which made the earth the center of the universe, but to his own chosen disciples he communicated the true doctrine that the sun occupies the center of the solar system, and that the earth is only one of the planets revolving around it. Like all the world's greatest sages, he seems to have taught only orally. A century elapsed before his doctrines were reduced to writing by Philolaus of Crotona, and it was still later before they were taught in public for the first time by Hicetas, or, as he is sometimes called, Nicetas, of Syracuse. Then the familiar cry of impiety was raised, and

the Pythagorean system was eventually suppressed by that now called the Ptolemaic, which held the field until it was overthrown by Copernicus, almost two thousand years later. Pliny tells us that Pythagoras believed the distances to the sun and moon to be respectively 252,000 and 12,600 stadia, or taking the stadium at 625 feet, 29,837 and 1,492 English miles; but there is no record of the method by which these numbers were ascertained.

After the relative distances of the various planets are known, it only remains to determine the scale of the system, for which purpose the distance between any two planets suffices. We know little about the early history of the subject, but it is clear that the primitive astronomers must have found the quantities to be measured too small for detection with their instruments, and even in modern times the problem has proved to be an extremely difficult one. Aristarchus of Samos, who flourished about 270 B. C., seems to have been the first to attack it in a scientific manner. Stated in modern language, his reasoning was that when the moon is exactly half full, the earth and sun as seen from its center must make a right angle with each other, and by measuring the angle between the sun and moon, as seen from the earth at that instant, all the angles of the triangle joining the earth, sun and moon would become known, and thus the ratio of the distance of the sun to the distance of the moon would be determined. Although perfectly correct in theory, the difficulty of deciding visually upon the exact instant when the moon is half full is so great that it cannot be accurately done even with the most powerful telescopes. Of course Aristarchus had no telescope, and he does not explain how he effected the observation, but his conclusion was that at the instant in question the distance between the centers of the sun and moon, as seen from the earth, is less than a right angle by $\frac{1}{30}$

part of the same. We should now express this by saying that the angle is 87 degrees, but Aristarchus knew nothing of trigonometry, and in order to solve his triangle, he had recourse to an ingenious, but long and cumbersome geometrical process which has come down to us, and affords conclusive proof of the condition of Greek mathematics at that time. His conclusion was that the sun is nineteen times further from the earth than the moon, and if we combine that result with the modern value of the moon's parallax, viz. : 3,422.38 seconds, we obtain for the solar parallax 180 seconds, which is more than twenty times too great.

The only other method of determining the solar parallax known to the ancients was that devised by Hipparchus about 150 B. C. It was based on measuring the rate of decrease of the diameter of the earth's shadow cone by noting the duration of lunar eclipses, and as the result deduced from it happened to be nearly the same as that found by Aristarchus, substantially his value of the parallax remained in vogue for nearly two thousand years, and the discovery of the telescope was required to reveal its erroneous character. Doubtless this persistency was due to the extreme minuteness of the true parallax, which we now know is far too small to have been visible upon the ancient instruments, and thus the supposed measures of it were really nothing but measures of their inaccuracy.

The telescope was first pointed to the heavens by Galileo in 1609, but it needed a micrometer to convert it into an accurate measuring instrument, and that did not come into being until 1639, when it was invented by Wm. Gascoigne. After his death in 1644, his original instrument passed to Richard Townley who attached it to a fourteen foot telescope at his residence in Townley, Lancashire, England, where it was used by Flamsteed in observing the diurnal parallax of Mars during its opposition in 1672.

A description of Gascoigne's micrometer was published in the *Philosophical Transactions* in 1667, and a little before that a similar instrument had been invented by Auzout in France, but observatories were fewer then than now, and so far as I know J. D. Cassini was the only person beside Flamsteed who attempted to determine the solar parallax from that opposition of Mars. Foreseeing the importance of the opportunity, he had Richer dispatched to Cayenne some months previously, and when the opposition came he effected two determinations of the parallax; one being by the diurnal method, from his own observations in Paris, and the other by the meridian method from observations in France by himself, Römer and Picard, combined with those of Richer at Cayenne. This was the transition from the ancient instruments with open sights to telescopes armed with micrometers, and the result must have been little short of stunning to the seventeenth century astronomers, for it caused the hoary and gigantic parallax of about 180 seconds to shrink incontinently to ten seconds, and thus expanded their conception of the solar system to something like its true dimensions. More than fifty years previously Kepler had argued from his ideas of the celestial harmonies that the solar parallax could not exceed 60 seconds, and a little later Horrocks had shown on more scientific grounds that it was probably as small as 14 seconds, but the final death-blow to the ancient values ranging as high as two or three minutes came from these observations of Mars by Flamsteed, Cassini and Richer.

Of course the results obtained in 1672 produced a keen desire on the part of astronomers for further evidence respecting the true value of the parallax, and as Mars comes into a favorable position for such investigations only at intervals of about sixteen years, they had recourse to observations

of Mercury and Venus. In 1677 Halley observed the diurnal parallax of Mercury, and also a transit of that planet across the sun's disk, at St. Helena, and in 1681 J. D. Cassini and Picard observed Venus when she was on the same parallel with the sun, but although the observations of Venus gave better results than those of Mercury, neither of them was conclusive, and we now know that such methods are inaccurate even with the powerful instruments of the present day. Nevertheless, Halley's attempt by means of the transit of Mercury ultimately bore fruit in the shape of his celebrated paper of 1716, wherein he showed the peculiar advantages of transits of Venus for determining the solar parallax. The idea of utilizing such transits for this purpose seems to have been vaguely conceived by James Gregory, or perhaps even by Horrocks, but Halley was the first to work it out completely, and long after his death his paper was mainly instrumental in inducing the governments of Europe to undertake the observations of the transits of Venus in 1761 and 1769, from which our first accurate knowledge of the sun's distance was obtained.

Those who are not familiar with practical astronomy may wonder why the solar parallax can be got from Mars and Venus, but not from Mercury, or the sun itself. The explanation depends on two facts. Firstly, the nearest approach of these bodies to the earth is for Mars 33,870,000 miles, for Venus 23,654,000 miles, for Mercury 47,935,000 miles and for the sun 91,239,000 miles. Consequently, for us Mars and Venus have very much larger parallaxes than Mercury or the sun, and of course the larger the parallax the easier it is to measure. Secondly, even the largest of these parallaxes must be determined within far less than one-tenth of a second of the truth, and while that degree of accuracy is possible in measuring short arcs, it is quite unat-

tainable in long ones. Hence one of the most essential conditions for the successful measurement of parallaxes is that we shall be able to compare the place of the near body with that of a more distant one situated in the same region of the sky. In the case of Mars that can always be done by making use of a neighboring star, but when Venus is near the earth she is also so close to the sun that stars are not available, and consequently her parallax can be satisfactorily measured only when her position can be accurately referred to that of the sun, or, in other words, only during her transits across the sun's disk. But even when the two bodies to be compared are sufficiently near each other, we are still embarrassed by the fact that it is more difficult to measure the distance between the limb of a planet and a star or the limb of the sun than it is to measure the distance between two stars, and since the discovery of so many asteroids, that circumstance has led to their use for determinations of the solar parallax. Some of these bodies approach within 75,230,000 miles of the earth's orbit, and as they look precisely like stars, the increased accuracy of pointing on them fully makes up for their greater distance, as compared with Mars or Venus.

After the Copernican system of the world and the Newtonian theory of gravitation were accepted it soon became evident that trigonometrical measurements of the solar parallax might be supplemented by determinations based on the theory of gravitation, and the first attempts in that direction were made by Machin 1729 and T. Mayer in 1753. The measurement of the velocity of light between points on the earth's surface, first effected by Fizeau in 1849, opened up still other possibilities, and thus for determining the solar parallax we now have at our command no less than three entirely distinct classes of methods which are known respectively as the trigonometrical, the grav-

itational and the photo-tachymetrical. We have already given a summary sketch of the trigonometrical methods, as applied by the ancient astronomers to the dichotomy and shadow cone of the moon, and by the moderns to Venus, Mars and the asteroids, and we shall next glance briefly at the gravitational and photo-tachymetrical methods.

* * * * *

The theory of probability and uniform experience alike show that the limit of accuracy attainable with any instrument is soon reached; and yet we all know the fascination which continually lures us on in our efforts to get better results out of the familiar telescopes and circles which have constituted the standard equipment of observatories for nearly a century. Possibly these instruments may be capable of indicating somewhat smaller quantities than we have hitherto succeeded in measuring with them, but their limit cannot be far off because they already show the disturbing effects of slight inequalities of temperature and other uncontrollable causes. So far as these effects are accidental they eliminate themselves from every long series of observations, but there always remains a residuum of constant error, perhaps quite unsuspected, which gives us no end of trouble. Encke's value of the solar parallax affords a fine illustration of this. From the transits of Venus in 1761 and 1769 he found 8.58 seconds in 1824, which he subsequently corrected to 8.57 seconds, and for thirty years that value was universally accepted. The first objection to it came from Hansen in 1854, a second followed from Le Verrier in 1858, both based upon facts connected with the lunar theory, and eventually it became evident that Encke's parallax was about one-quarter of a second too small. Now please observe that Encke's value was obtained trigonometrically, and its inaccuracy was never suspected until it was revealed by gravitational methods

which were themselves in error about one-tenth of a second and required subsequent correction in other ways. Here then was a lesson to astronomers who are all more or less specialists, but it merely enforced the perfectly well known principle that the constant errors of any one method are accidental errors with respect to all other methods, and therefore the readiest way of eliminating them is by combining the results from as many different methods as possible. However, the abler the specialist the more certain he is to be blind to all methods but his own, and astronomers have profited so little by the Encke-Hansen-Le Verrier incident of thirty-five years ago that to-day they are mostly divided into two great parties, one of whom holds that the parallax can be best determined from a combination of the constant of aberration with the velocity of light, and the other believes only in the results of heliometer measurements upon asteroids. By all means continue the heliometer measurements, and do everything possible to clear up the mystery which now surrounds the constant of aberration, but why ignore the work of predecessors who were quite as able as ourselves? If it were desired to determine some one angle of a triangulation net with special exactness, what would be thought of a man who attempted to do so by repeated measurements of the angle in question while he persistently neglected to adjust the net? And yet, until recently astronomers have been doing precisely that kind of thing with the solar parallax. I do not think there is any exaggeration in saying that the trustworthy observations now on record for the determination of the numerous quantities which are functions of the parallax could not be duplicated by the most industrious astronomer working continuously for a thousand years. How then can we suppose that the result properly deducible from them can be materially

affected by anything that any of us can do in a lifetime, unless we are fortunate enough to invent methods of measurement vastly superior to any hitherto imagined? Probably the existing observations for the determination of most of these quantities are as exact as any that can ever be made with our present instruments, and if they were freed from constant errors they would certainly give results very near the truth. To that end we have only to form a system of simultaneous equations between all the observed quantities, and then deduce the most probable values of these quantities by the method of least squares. Perhaps some of you may think that the value so obtained for the solar parallax would depend largely upon the relative weights assigned to the various quantities, but such is not the case. With almost any possible system of weights the solar parallax will come out very nearly $8.809'' \pm 0.0057''$, whence we have for the mean distance between the earth and sun 92,797,000 miles with a probable error of only 59,700 miles; and for the diameter of the solar system, measured to its outermost member, the planet Neptune, 5,578,400,000 miles.

WILLIAM HARKNESS.

WASHINGTON.

THE BALTIMORE MEETING OF THE AMERICAN SOCIETY OF NATURALISTS.

THE thirteenth annual meeting of *The American Society of Naturalists* was held at Baltimore during the Christmas vacation. Considering that Baltimore is the southern limit where meetings may be held by the Society, the attendance was large, forty to fifty members being present.

The first session was called to order by the President, Professor Charles S. Minot of the Harvard Medical School, at 2 p. m. on Thursday, December 27th.

A quorum being present, the Society at once proceeded to the transaction of business. The committee appointed in 1893 to

obtain, if possible, the removal of the duty on scientific instruments reported that although they had succeeded in obtaining the coöperation of most of the leading scientific men, yet the inception of the movement had been so delayed that the Gorman Bill was already being considered by the Senate before the petitions could be presented to the House.

The following resolution recommended by the committee was then adopted: "Inasmuch as the repeal of the present iniquitous duty on scientific instruments is imperatively needed by the interests of the country, we recommend that a committee be appointed to present our just demands to the President, to the Chairman of the Committee on Finance of the Senate and the Chairman of the Committee of Ways and Means of the House of Representatives, and to take such other steps as may be practicable to secure the immediate repeal of the duty."

The report of the committee on the revision of the Constitution and By-Laws was unanimously adopted. By the new constitution *The American Society of Naturalists* encourages the formation of other societies of similar name and object in other parts of the country and invites other societies whose chief object is the encouragement of the study of Natural History to become affiliated with it. The affiliated societies shall have a common place and time of meeting with the American Society of Naturalists, the expenses of which are to be paid from a common treasury supplied from a common fee. The records of the secretaries of the different societies are also to be published at common expense.

The discussion upon *Environment in its Influence upon the Successive Stages of Development and as a Cause of Variation*, took place in the Physical Lecture Room, Thursday afternoon. It was opened by four papers and followed by remarks by Professors Cope

and Hyatt, Dr. Dall, Dr. C. V. Riley and others.

Professor Osborn, of Columbia College, in opening the discussion, observed that naturalists were reacting from the discussion of *theories* towards the renewed inductive and experimental study of the factors of Evolution. This was due to the feeling that the prolonged discussion led by Spencer and Weismann had assumed a largely deductive character and would not lead to any permanent results. The inductive reaction had taken two directions: first towards the exact study of Variation, and second towards experimental Evolution. As regards Variation we should not expect to form any laws so long as variations were considered *en masse* without regard to the past and present history of the organisms studied. That organisms vary with their environment is a truism. What we need is a clearer conception and interpretation of this relation as a basis for experimental study in the laboratory and in the field. The first misconception to be removed is that which has sprung up from the misuse of the terms Heredity and Variability. Nägeli pointed out many years ago as Weismann and Hurst have insisted more recently that Heredity includes one phenomenon seen from two sides which may conveniently be termed Repetition and Variation. A large number of the variations recorded by Bateson, for example, are simple repetitions of ancestral structure, and every new variation is to be regarded as the expression of hereditary forces working under new conditions. The first object of investigation is to decide the *time of origin* of a variation, first in race history, second in individual history. Variations which arise as practical repetitions of past experience may conveniently be termed '*palingenic*,' while those which are new to the organism may be termed '*cenogenic*.' As regards individual history the most important question is to determine

whether a variation is merely '*ontogenic*,' that is springing up in the course of individual development from some disturbance of the hereditary mechanism, or '*phylogenic*' and constant as distinguished by Nägeli. From recent study of palingenic variation we must recast our conception of Heredity especially in view of the remarkable researches of Cunningham upon the color, and of Agassiz, Giard and Filhol upon the symmetry of the flat fishes (*Pleuronectidæ*). These characters of enormous antiquity, summoned as it were from the vasty deep, reveal the law that repetition or variation in ontogeny depend largely upon repetition or variation in environment, that for many of the most fundamental characters, development and environment are inseparable and all theories which tend to separate the two are untenable. As regards cenogenic variations or those which are new in the experience of the organism, the distinction between ontogenic variations, or what are commonly called acquired characters, and phylogenic variations is also of pressing importance. The organism may be compared to a clock, keeping regular time upon a base; if the base is tilted slightly the clock may continue to tick but it may not keep the same time; if after the lapse of a long period the base is restored to its original position the clock will tick in correct time as before. This thought shows that the conditions which have been demanded as crucial tests of the permanent phylogenic influence of environment upon organisms will be very difficult to fulfill in experiment—when the repetition of a mesozoic environment is found to produce a repetition of a mesozoic structure. Experiment should now be directed separately upon each of the four stages of development (germ cell, fertilization, embryonic, larval and adult) and then withdrawn, and putting together the results of all the work which has been recently done of this kind

we find three classes of variation phenomena coming to the surface; first '*palingenic* variations,' second '*saltations*,' third '*ontogenic* adaptations' (Haeckel); fourth a class of '*phylogenic* variations' which have been termed '*mutations*' by some paleontologists. We are so far from a solution of the working causes of these four classes of variation that it seems best to consider that we are on the threshold of the Evolution Problem, to take an entirely agnostic or doubtful position as to all the prevalent theories, and press forward in strictly inductive search for laws which may not be forthcoming until the next century.

Professor Edmund B. Wilson, of Columbia College, followed with a discussion of the influence of the environment on the early stages of embryonic development. That a change of external conditions, such as temperature, chemical nature of the medium and the like, causes changes in the rate or form of development has long been a familiar fact, but we have only recently come to perceive clearly how significant are the changes thus brought about and how vital is the part played by the environment in all development, whether pathological or normal. For if a changed mode of development is the '*result*' of a change of environment, the normal development must in exactly the same sense be the '*result*' of the normal environment, *i. e.*, in both cases we are dealing with a definite physiological response of the idioplasm to external conditions. The facts both of normal and of experimental embryology demonstrate the justness of this point of view. The experiments of Pflüger, Driesch, Roux and others show, for instance, that the forms of cleavage may be profoundly altered by mechanical means, and indicate that some of the normal fundamental cleavage-forms are the direct result of mechanical conditions, such as the shape of the egg, pressure of the membranes, surface tensions between the blastomeres, and the

like. Temperature has a profound effect not only on the rate of development, but also on its form. Thus Driesch showed that the eggs of sea-urchins when incubated at a temperature slightly above the normal undergo remarkable changes. The form of cleavage may be considerably altered (without affecting the end result of development), and the gastrulation may be profoundly affected. In some cases 'exogastrulæ' are formed, the archenteron being turned out instead of in, and these undergo all the normal differentiations of the *Pluteus*, though they ultimately perish since the alimentary canal is turned inside out and the larvæ are incapable of taking food. Other physical agents such as gravity have been shown to have a profound effect on development, determining the position of roots and branches in hydroids (Loeb), or even the polar differentiation of the egg as in the frog (Pflüger, Born, etc.).

The most remarkable and significant examples of environmental influence are, however, found in the effect of change in the chemical environment. In the case of sea-urchins Ponchet and Chabry found that in sea-water deprived of calcareous matter the *Pluteus* larva is unable to develop its spicular skeleton, and Herbst showed that the same result was produced by a very slight excess of potassium chloride in the water even though the normal amount of calcareous matter were present. In both cases the larvæ not only fail to develop spicules, but are unable to produce the characteristic ciliated arms. Thus arises a larva having a simple ciliated belt and very similar to a young *Tornaria*. This is a very instructive case; for it shows in the first place that a definite character (formation of the skeleton) has a fundamental though very subtle relation to the external environment, and in the second place, that this relation indirectly extends to other characters (ciliated arms) that follow upon the development of

the first character. Such cases pave the way to a rational conception of epigenesis, by showing the multiplication of effects in ontogeny and the complicated results that may follow from a single and apparently insignificant condition of the environment.

Even more striking results are those obtained (by Herbst) by the addition of a minute percentage of lithium chloride to the sea-water. The primary result is to cause exogastrulation (like the effect of raised temperature). Beyond this, however, the entoderm area (*i. e.*, archenteric region) often becomes abnormally large. The entoderm may then be reduced to a mere knob consisting of only a few cells, or may even disappear altogether so that a blastula is formed that *consists entirely of entoderm!* This extraordinary result, if it can be accepted, shows that even so fundamental a process as the differentiation of the germ-layers stands in a vital relation to the chemical environment. It is a revelation of the importance of environmental influences in development and it shows that we must readjust our conceptions not only of development but also of inheritance, of which development is an expression. Our attention has been focussed too closely upon the formal morphological aspect of development which we have regarded too largely as the result of a pre-organized germ-plasm operating like a machine. Embryological development must be thoroughly re-examined from a physiological point of view, full weight being given to the essential part played by the environment. This point of view in no way sets aside the necessity of assuming a specifically organized germ-plasm for every species as the basis of inheritance. It simplifies the problem, however, and opens the way for further investigation, which is practically barred by the artificial and formal theories of development advocated by Roux and Weismann.

The third paper read was by Professor

W. K. Brooks, of the Johns Hopkins University. The subject was: *An Intrinsic Error in the Theories of Galton and Weismann*. It will be published in full later. The principal point taken was against the theory of variation springing from a mixture of ancestral characters. It was shown that many lines of descent may arise from a very small number of parents and represent a slender thread, consisting of very few strands, many individuals of the same species having an identical remote ancestry. In other words, sexual environment instead of being unlimited is very narrow, and as we pass backwards the number of ancestors increases rapidly for a number of generations, and then decreases instead of increasing indefinitely. The causes of variation are therefore to be sought rather in modern conditions of organisms than in the remote past.

Dr. C. Hart Merriam, of the United States Department of Agriculture, contributed an exhibition and discussion of a beautiful series of mammal and bird types exhibiting protective coloring and a number of dynamic variations. The origin of protective colors is to be sought in fortuitous variation preserved by selection. The theory of the direct action of environment in modifying color as in the bleached types of the desert regions is not borne out by observations and is disproved in the case of nocturnal types. A second and distinct class of facts comes under the head of Dynamic Variation, and to this class we refer to modifications of the beak, of the feet and limbs as due primarily to the habits and activities of the animals themselves.

At the close of the afternoon session, Professor E. B. Wilson, of Columbia College, exhibited by means of the stereopticon, lantern slides, prepared from photographs taken from sections, illustrating the cytological changes during maturation, fecundation, and segmentation. The different effects of the various killing, fixing and stain-

ing agents upon the ultimate details of cell-structure, were admirably brought out.

At eight o'clock the Society had the pleasure of listening to Professor William Libbey, of Princeton, who told of his experiences during *Two Months in Greenland*. The lecture was illustrated by a large number of magnificent views of Polar Scenery.

After the lecture the members were entertained by the authorities of the Johns Hopkins University and the citizens of Baltimore at a most pleasant assembly in McCoy Hall.

The Society reassembled at nine o'clock on Friday morning, Dec. 28th.

Officers for the year 1895 were chosen as follows:

President—Professor E. D. Cope, University of Pennsylvania.

Vice Presidents—Professors Wm. Libbey, Jr., Princeton University; W. G. Farlow, Harvard University; C. O. Whitman, Chicago University.

Secretary—Professor H. C. Bumpus, Brown University.

Treasurer—Doctor E. G. Gardiner, Boston, Mass.

Committee-at-Large—Professors E. B. Wilson, Columbia College; W. H. Howell, Johns Hopkins University.

The following persons were elected to membership in the Society:

William Ashmead, U. S. Dept. Agriculture, Washington; Severance Burrage, Mass. Inst. Tech., Boston; W. E. Castle, Harvard University; H. E. Chapin, University of Ohio, Athens, Ohio; J. E. Humphrey, Johns Hopkins University; M. M. Metcalf, Woman's College, Baltimore; H. C. Porter, University of Pennsylvania; W. H. C. Pynchon, Trinity College, Hartford; Charles Schuchert, U. S. National Museum; Norman Wyld, late of Bristol, England.

The report of the Treasurer showing a balance of somewhat over \$200 was accepted by the Society.

The Society, on motion of Professor Bumpus, appropriated a sum not to exceed \$130 to equip the American table at the Naples Station with proper microtomes, and a committee of three was appointed to attend to this matter.

Professor J. S. Kingsley detailed a 'bibliographical project' originating with Professor G. W. Field of Brown University. This proposes to put into the hands of workers in zoölogy a bibliography of current literature, in such a form as to be readily accessible, the latter to be readily combined with the earlier, and to present the matter both as to subjects and as to authors. By a vote of the Society, a committee of five was appointed to consider this 'project' and to report in print both in *SCIENCE* and in *The American Naturalist*.

President Gilman, in a very pleasant and cordial way, then welcomed the members of the visiting societies to Baltimore, speaking on behalf of the authorities of the Johns Hopkins University and of the citizens of Baltimore.

President Minot chose for the subject of his address '*The Work of the Naturalist in the World.*' The object of the naturalist is to discover the truth about nature and to publish the results of his work to the world. The conditions of success are readily to be observed. First and foremost is truth. The naturalist's first business is to get at the truth, and the obstacles which stand most prominently in his way are: (1) the limitations of his own abilities, and (2) the limitations of accessories for carrying on his work. The naturalist must observe, experiment and reason, and his training must necessarily be along these lines. Experimentation is necessarily more difficult than observation, for in the former case the naturalist asks why, not how. The great work of the future, as is already being shown, is to be done by the experimenters.

Our notion of causation is still in a very rudimentary condition.

Again, the reasoning faculty is one of our weakest points. The naturalist must learn to carefully distinguish between discussion and controversy, and while being led and taught to indulge freely in the former with all the intelligence at his command, he must also be taught to avoid the latter.

The naturalist is naturally exposed to many evils, such as this matter of controversy, which tend to cause him to depart from his proper mission, viz., of getting at the truth. He is especially likely to be led astray by impatience to get results. Preliminary communications are a very great as well as a very prevalent evil. The opinion of the speaker was very pronouncedly adverse to this form of publication. The greed for priority leads many even fine workers far astray.

The tendency to speculate is a third evil, and this has perhaps reached its culmination in the doctrines of Weismann. Another evil is the one which leads us to accept too readily simple and well finished conceptions. Herbert Spencer furnishes us with an illustrious example of the effects of this.

In the matter of publication, four classes may be distinguished: (1.) *Original Memoirs*; (2.) *Handbooks*; (3.) *Text Books*; (4.) *Bibliographies*. The last three are important both in form and in the matter. The first are like digestive organs. It is their function to assimilate crude facts and render them digestible. Advice to prune and digest such matter for publication is much needed. Details not bearing directly upon the subject should be carefully excluded. Most original papers could be 'boiled down' to one half, and some even to one tenth of the amount that is really published. The English write best and this may be owing to the example of Huxley. The Germans and Americans who copy after

them come next, and the French are the greatest sinners in the matter of verbiage.

The effect of the work of the naturalist upon his own character is especially shown in his optimism. Literary men seem much inclined to grow pessimistic. This point is well illustrated by a comparison of the recently published letters of Asa Gray and of James Russell Lowell. Lowell's letters show increasing pessimistic views toward the end of his life, while those of Gray remain uniformly optimistic. Something of this was undoubtedly due to the different temperaments of the two men, but much was also due to the different nature of their work. Gray could always see new things unfolding before him.

One drawback in the naturalist's life is his comparative loneliness and isolation. Seldom has he in his own neighborhood another interested in the same particular line as himself. Reunions of naturalist societies, such as those at the time meeting in Baltimore, counteract this to a considerable extent, but there is need of even greater affiliation.

The influence of the naturalist upon mankind in the way of teaching them competence had not been considered sufficiently. In political questions competency comes in, and the solution of much of our present trouble lies not so much in restricting the right to vote as it does in restricting the right to become a candidate. We, as naturalists and as citizens, should uphold competence. Our schools, even the best of them, judging by their results, do not educate properly. The naturalist should see to it that our schools educate, with science in its proper place. It is the duty of the naturalist to advance the development of the university. The schools use elementary knowledge to advance the mind in acquisitiveness, and the college uses advanced knowledge in the same way, but the university attempts to advance the mind in

independent work, to develop and discipline originality.

To carry on its proper work the university needs a large endowment, at least \$10,000,000. It is not possible to teach zoölogy unless the proper instruments and books are provided. The university, above all, needs proper professors. The qualifications of a professor in a university should be: (1) the ability to carry on original researches himself, and (2) to train others to carry out original work.

The annual discussion on '*Laboratory Teaching of Large Classes*' followed Professor Minot's address. Professor Alpheus Hyatt, of the Boston Society of Natural History, introduced the subject somewhat as follows:

Teaching has two objects in view: (1) to train the faculties of individuals, and (2) to increase the store of information. The importance in laboratory teaching of bringing the pupil into contact with the organisms themselves is absolutely necessary. The term, 'large classes,' is relative. It may mean twenty, thirty, forty, up to several hundred. In teaching large classes, there must be taken into account the matter of division into sections, rooms, assistants, apparatus, etc. The first point to be insisted upon is the matter of personal contact between the pupils and the instructors. In experience with Boston teachers, the classes numbered five hundred. Tables were provided for the whole number, and on these tables were placed the trays of specimens on which the exercise was to be given. The specimens were thus arranged before the exercises by assistants. The lecturer then proceeded to demonstrate the various points upon his own specimens, and the pupils followed him by working out the same points on the specimens in the tray. The specimens kept the lecturer down to his subject and also kept the pupils at work. Of course the field was necessarily limited.

The initial expense for providing the material was small, being about \$10 for geology, \$15 for botany, and \$25 for zoölogy. Diagrams and crayon sketches, magnifying glasses, and various methods of a simple kind were made use of. These methods were afterwards used with smaller sections with even more satisfactory results. Examinations were given to test the pupils' proficiency, not only in knowledge of the subject but also of methods of study. For this purpose test objects were given the pupils to examine and describe. At the close of his paper, Professor Hyatt exhibited some specimens of these examinations.

Professor H. C. Bumpus, of Brown University, spoke upon the subject from the zoölogical point of view. The value of laboratory work depends largely upon good material, which should be supplied in abundance and in excellent condition. At the present time there is no excuse for supplying poor or scanty material, since abundance of excellent material can be obtained at small cost. The importance of having the best dissections and best drawings obtainable in the laboratory itself cannot be overestimated. It does not induce the laziness and attempts at shirking that seem to be the fear of so many teachers. If the student desires to copy a fine dissection he is to be encouraged to do so, and any teacher can readily detect the sketches copied from a chart or diagram. The speaker said also that he had found it an excellent plan in certain difficult cases to supply blanks on which the outlines of important structures were laid down, the details to be added by the pupil. A printed outline of the order of work, directions for manipulation, and questions to be answered from the specimens are a great help. The need of competent assistants is obvious.

The botanical side of the question was considered in a paper by Professor W. F. Ganong, of Smith College. The experience

given was obtained in managing classes of about 200 men at Harvard, and the plan given was worked out under the guidance of Professor G. L. Goodale. The conditions under which the instruction was undertaken were: (1) The classes were too large for individual teaching by the instructor; (2) laboratory hours must be adjusted to other academic work, to insufficient accommodations, and sometimes even to yet other considerations; (3) many students of diverse attainments must be taught how to work and to think scientifically, and must be kept progressing together through the stages of a logically graded course, and (4) large quantities of special material must be provided for at unfavorable seasons.

In conducting such classes competent assistants were necessary, each to have not more than twenty men under him, and these were to remain under his special charge throughout the course. Such assistants may be readily recruited in any large university where there are special students doing advanced work. The assistants met the instructor to talk over plans and details of coming work. Uniformity of plan was insisted upon, but details of method were left to the assistant. The instructor did not devote himself to any one section, but visited each one as often as was possible. Weekly guides were printed for the use of the student, indicating the points to be studied, their relative importance, and any necessary information given. They were intended to supply just enough data to enable the student to progress to correct conclusions.

The materials required were arranged in the course, so that in the winter such things as could be grown easily or procured out of doors, as seeds, seedlings and buds, came first, and then followed the succession of opening buds, leaves, flowers and fruit made accessible by the advance of spring. In other words, the time of giving the course and the

grouping of the subjects was so arranged that the material for each subject was in proper condition when it came before the class. Some of the weekly guides accompanied the paper, for examination.

Discussions were presented by Professors H. W. Conn, Marcella O'Grady, E. S. Morse and C. S. Minot, and the additional fact was brought out that a good synoptic collection was a desirable feature of the laboratory equipment, in order that the pupil might not have too narrow a view of each group of organisms, such as he is likely to carry away from the study of a single type.

After passing a vote of thanks to the authorities of the University, the citizens of Baltimore and the University Club for the hospitality extended to it, the Society adjourned.

The annual dinner of the affiliated Societies took place at 'The Stafford' at 7:30 on Friday evening. No set toasts were given, but informal speeches formed a very pleasurable close to this reunion.

W. A. SETCHELL, *Secretary*.

YALE UNIVERSITY.

THE PRINCETON MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION.

THE third annual meeting of *The American Psychological Association* was held at Princeton College on Thursday and Friday, December 27th and 28th, under the presidency of Professor William James, of Harvard University. Psychology is the youngest and likewise one of the most vigorous of the sciences. Although the Association is small, consisting of those only who are actively engaged in psychological investigation, and the members are widely scattered, there were sixteen papers read, exclusive of those presented in the absence of their authors. Indeed, the only drawback to the pleasure of the meeting was the fact that the program was so crowded that there was not sufficient time for discussion and

social intercourse. The short intervals between the meetings were, however, pleasantly filled, owing to the hospitality of President Patton and Professor Baldwin, and the excellent accommodations of the Princeton Inn.

The Association was welcomed to Princeton by President Patton in a fitting address in which he alluded to the importance of such meetings, not only for the advancement of science, but also for the cultivation of inter-university friendliness, to the death and life-work of President McCosh, and to the prominent place always given to philosophy and psychology at Princeton.

The papers presented covered a wide range of psychological topics. Experimental psychology proper was not so fully represented as in the Philadelphia and New York meetings, owing to the detention of several members, but all the communications were strictly scientific in method.

The first paper, *Minor Studies and Apparatus*, by Professor Sanford, was, indeed, of purely experimental character, coming from Clark University, where President Hall has given such a prominent place to experimental psychology. Professor Sanford first showed charts demonstrating that the retinal fields for color are relatively smaller in the case of children than in the case of adults. In the second study he reported experiments on the accuracy with which an observer can distinguish by different senses which of two stimuli is first presented. A flash of light is perceived relatively earlier than a sound—contrary to results formerly published by Exner. In a third study primary memory was investigated. In a fourth study questions were asked students concerning the confusion of related ideas, for example:—How do you distinguish your right from your left hand? How do you call up a forgotten name? How do you collect the attention? What were your favorite games when a child? What is the earliest

thing you can remember and how old were you? The distinction between motor and sensory types and other psychological questions were discussed in connection with the answers, and the method of securing mental statistics by asking questions was criticized. In conclusion, an instrument was shown for presenting objects alternately to each eye, and charts and photographs illustrating illusions of size, Listing's Law and the Horopter. These studies will be published in the forthcoming number of the *American Journal of Psychology*.

Professor Ormond, Professor Baldwin and others took part in the discussion that followed the reading of the paper. The discussion of the different papers was of nearly as great interest as the papers themselves, but to report it would carry us too far into details.

The second paper was on *The Psychic Development of Young Animals and its Physical Correlation*, by Professor T. Wesley Mills, of McGill University. The speaker emphasized the importance of comparative and genetic psychology—that is the study of the mental life of the lower animals and of children. He had observed the dog, cat, rabbit, guinea-pig and birds. They were watched from their birth, and notes were made several times during the day. The method was emphasized rather than the results, which will be published later.

Following Professor Mills' paper was one *On the Distribution of Exceptional Ability*, by Professor Cattell. The speaker explained how he had selected the 1,000 most eminent men by an objective method, and how this enabled him to measure and express numerically their mental traits. Curves were shown giving the time and racial distribution of great men. These demonstrate the rise and fall of leading tendencies in the past, and enable us, to a certain extent, to predict the course of civilization in the future.

Dr. A. Macdonald, of the Bureau of Education, presented a report on *Sensitiveness to Pain*. He exhibited the instrument used and described his method for measuring sensitiveness to pain. Women are more sensitive than men in the ratio of 7:5. Men taken from the street are not half so sensitive to pain as professional men. Americans are more sensitive than Englishmen or Germans. The right-hand side of the body is less sensitive than the left-hand side. Some instruments for anthropometric tests were also exhibited and described.

At the close of the morning session Brother Chrysostom, of Manhattan College, read a paper on *Freedom of the Will*. This time-honored problem was discussed from the point of view of St. Thomas Aquinas, with due recognition of recent writers. The Catholic Church certainly deserves honor for finding or putting modern science in the works of the great Schoolman.

The afternoon session was opened by the longest and most carefully prepared paper of the meeting, *Consciousness of Identity and So-Called Double Consciousness*, by Professor Ladd, of Yale University. Professor Ladd began by defining identity in material things and in minds. Changes heighten rather than diminish the consciousness of identity. A metaphysical ego is not needed—minds vary in their unity and reality. Double consciousness and hypnotic states should be treated in their relations to normal mental life, as it is not likely that the principle of continuity is violated in this case. Psychological automatism should be carefully studied—a man is not only that of which he is conscious. We can consider our automaton as well as our ego; one or the other may be predominant; they may be in conflict or act in coöperation. The automaton is evident in our daily life—in games, in dreams, in dramatic composition and acting, in prophecy. Ethically considered, a man is usually two or three,

rather than one—hence the categorical imperative of Kant. The sanest minds are at times divided into two or more selves, as much as are the most extreme cases of hypnotic or pathological double-consciousness. Prof. Ladd's paper is included in his forthcoming work on Psychology, in the press of Charles Scribner's Sons. It excited much discussion and some criticism.

The remainder of the session was taken up by a paper on *A Preliminary Report and Observations on a Research into the Psychology of Imitation* by Professor Royce, of Harvard University. He began by noting the difficulty of defining imitation from other mental functions. He then described experiments now in progress in the psychological laboratory of Harvard University. An observer listens to a rhythmic series of taps which are later repeated or imitated by movements. The record was taken on a kymograph, and the impressions of the observers were noted and studied. The objective records have not been collated, but Professor Royce reported the subjective state as described by the observer, and its variations with different rhythms. In further discussion of the subject Professor Royce considered different kinds of imitation, and their relation to the rest of mental life and to the physical organism. The subject of imitation has recently become prominent and is evidently of the utmost importance in social psychology—not only the development of the child but also the thoughts, feelings and actions of men depend largely, if not chiefly, on imitation, and our theoretical knowledge has important practical applications.

The address of the President, Professor James, of Harvard University, occupied the evening session. The subject, *The Unity of Consciousness*, was treated with the speaker's unvarying clearness and literary skill. Professor James once said that metaphysics in a natural science 'spoils two

good things,' but no natural science, be it physics or psychology, can draw a sharp line between its facts and its philosophy. It is also worth noting that what the physicist considers part of his science may be regarded as metaphysics by the psychologist, and conversely. The question of the unity of consciousness is, perhaps, as much a part of scientific psychology as the doctrine of the conservation of energy is a part of the science of physics. Professor James' address was largely made up of a review of the various theories proposed to account for the principle of union in the mind when many objects, susceptible upon occasion of being known separately, are brought together in the mind and known all at once. The Associationists say that the 'ideas' of several objects 'combine.' The Anti-Associationists say that such a process of self-compounding of ideas is incomprehensible, and that they must be combined by a higher synthetic principle, the Soul, the Ego, or what not. The speaker expressed dissatisfaction with the both these views. He said that his own aversion to the doctrine of the 'Soul' rested on an ancient prejudice, of which he could give no fully satisfactory account to himself, and he complimented Professor Ladd, of Yale, for his continued loyalty to this unpopular principle. Even Professor Ladd in his book prefers to speak of 'Soul' by some paraphrase such as 'real spiritual being.' Within the bounds of the psychological professor the 'Soul' is not popular to-day. Professor James conceived his problem as that of how we can 'know things together,' and in the first half of his address he incidentally said a good deal about knowledge. To the popular mind all knowledge involves a sort of mutual presence or absence as regards the object and the mind, which is treated as very mysterious. Professor James expelled this mystery from most cases of knowledge. He found the mystery of presence or ab-

sence, however, to abide in one little fact, from which it cannot be driven, and that is the very smallest pulse of consciousness, which always is consciousness of change. The present moment is no fact of experience; it is only a mathematical postulate, and the minimum real experience gives us a passing moment, in which a going and a coming fact meet on equal terms, and what was is known in one indivisible act with what does not quite yet exist. This is the original type both of our knowing at all and of knowing of things together, according to the speaker. He said there was no use trying to explain it, for it was the fundamental element of all experience. But we might seek to determine the exact conditions that decide what particular objects should be known together, and to this inquiry the end of the address was devoted. Various physiological, psychological and purely spiritual theories of the conditions were reviewed, without the speaker saying which one he favored. He hoped, however, that his remarks might stimulate inquiry which should bear fruit at the meeting next year. He closed with a modification of one of the most important doctrines of his own book on psychology, which in that state of mind, subjectively considered, ought not to be called complex at all. He admitted them to be complex, but is as far as ever from allowing the complexity to be described in the usually accepted way of the Associational school. The address will be printed in full in the March number of *The Psychological Review*.

The morning session of the second day was taken up by five papers on pleasure, pain and the emotions, and in the afternoon when the papers of the program had been read, the discussion returned to this subject and was carried on with much eagerness to the moment of adjournment. The papers were *The Classification of Pleasure and Pain*, By Prof. Charles A. Strong, of the University

of Chicago; *A Theory of Emotions from the Physiological Standpoint*, by Prof. G. H. Mead, of the University of Chicago; *Desire*, by Dr. D. S. Miller, of Bryn Mawr College; *Pleasure and Pain Defined*, by Prof. S. E. Mezes, of the University of Texas; *Pleasure-Pain versus Emotion*, by Mr. H. R. Marshall.

It would not be easy to give an abstract of these papers that would be intelligible to men of science working in other departments—indeed, the most careful attention was demanded of the audience. The kind of psychology presented is a development of descriptive psychology which may be called analytic psychology—a subject best represented in English by Dr. Ward's able but difficult article on *Psychology*, in the *Encyclopædia Britannica*. The question of the emotions and their expression has recently become prominent in psychological discussion—witness the articles on the subject by Professors James, Baldwin and Dewey in the last three numbers of the *Psychological Review*. Professor James' original theory that the mental state is rather the result of the 'expression' than that the expression is caused by the mental state is pretty well made out. The theory, to put the matter most bluntly, says that, "we feel sorry because we cry, angry because we strike, afraid because we tremble, and not that we cry, strike or tremble, because we are sorry, angry or fearful, as the case may be." Darwin's work, for example, should not be called *The Expression of the Emotions*. The movements are not caused by the emotions, but are aroused reflexly by the object, and are or have been useful. Thus the animal in the presence of its enemy may feign death or run away as will best contribute to its chances of escape, and a man may be 'paralyzed' by fear or flee according to circumstances. A man sneers because his ancestors were preparing to bite. The mental emotion results from

movements and other changes in the body, being largely due to altered blood supply and the like.

Professor Strong's paper treated especially the classification of pains, reviewing the evidence in favor of special nerves for pain and the distinction between pain and distress (the German *Schmerz* and *Unlust*). Mr. Mead emphasized the importance of vaso-motor changes for pleasure and pain, attributing pleasure to increased blood supply and assimilation. Dr. Miller argued that desire is the essence of pleasure, and Mr. Marshall discussed the relations of pain, pleasure and emotion. It is interesting to note how even descriptive and analytic psychology is influenced by a psycho-physical point of view. Professor James aptly concluded the discussion by saying that such papers make us feel that we are in 'the place where psychology is being made.'

At the opening of the fifth and concluding session Professor Newbold read a paper entitled *Notes on the Experimental Production of Illusions and Hallucinations*. He reported that in twenty-two cases out of eighty-six tried, he had produced illusions by causing the patient to gaze into a transparent or reflecting medium, such as water, objects of glass and mirrors. The phantasm usually appeared within five minutes, was preceded by cloudiness, colors or illumination of the medium, and varied from a dim outline to a brilliantly colored picture. These were often drawn from the patient's recent visual experience, but were often unrecognized and sometimes fantastic. Successive images were usually related, if at all, by similarity, but often no relation was discoverable. The image was often destroyed by movements of the medium and by distracting sensory impressions and motor effort. The speaker was not inclined to regard the phantasms of the glass as demonstrating the existence of subconscious visual automa-

tisms, but rather as illusions of the recognized types. But he was not prepared to deny that visual automatism might in some cases exist and be traced in such phantasms.

Mr. Griffing, of Columbia College, described *Experiments on Dermal Pain*. The pressure just causing pain (in kg) was for boys 4.8, for college students 5.1, for law students 7.8, for women 3.6. Experiments were also described giving the relations of area and duration and of velocity and mass for the pain threshold. These latter experiments are of special interest as determining the correlation of quantities followed by a given mental result.

The third paper of the session and last of the meeting was on *Recent Advances in the Chemistry and Physiology of the Retina*, by Mrs. Franklin, of Baltimore, who gave an account of the recent experiments by Professor König on the absorption spectrum of the visual purple of the retina, and of her own experiments which demonstrated that the fovea is color-blind for blue. The recent experiments on vision, largely carried out in the laboratories of Berlin, are of great importance, and make all the older theories of color-vision inadequate. The theory proposed by Mrs. Franklin is undoubtedly more satisfactory than any other, but even her theory meets difficulties in these new facts.

At the business meeting of the Association Professor Cattell (Columbia) was elected President, and Professor Sanford (Clark), Secretary. Several new members were elected and a new constitution was adopted. Under this constitution a council of six members is prescribed, and Professors Ladd (Yale), Cattell (Columbia), James (Harvard), Baldwin (Princeton), Dewey (Chicago), and Fullerton (Pennsylvania) were elected. Probably the most important business before the meeting was the invitation of the American Society of Naturalists offering affiliation. It was de-

decided to meet next year, if possible, at the same time and place as the Naturalists, and the Council was given power to decide the question of a closer affiliation.

J. McKEEN CATTELL,
Secretary for 1894.

COLUMBIA COLLEGE.

CURRENT NOTES ON ANTHROPOLOGY, NEW SERIES—I.

THE 'MISSING LINK' FOUND AT LAST.

No publication of late date is likely to excite more interest than a quarto of forty pages which has just been issued from the local press of Batavia, with the title, '*Pithecanthropus Erectus. Eine Menschenähnliche Uebergangsform aus Java.* Von Eug. Dubois, Militärarzt der Niederland. Armee.'

This noteworthy essay contains the detailed description of three fragments of three skeletons which have been found in the early pleistocene strata of Java, and which introduce to us a new species, which is also a new genus and a new family, of the order of primates, placed between the *Simiidae* and *Hominidae*,—in other words, apparently supplying the 'missing link' between man and the higher apes which has so long and so anxiously been awaited.

The material is sufficient for a close osteological comparison. The cubical capacity of the skull is about two-thirds that of the human average. It is distinctly dolichocephalic, about 70°—and its *norma verticalis* astonishingly like that of the famous Neanderthal skull. The dental apparatus is still of the simian type, but less markedly so than in other apes. The femora are singularly human. They prove beyond doubt that this creature walked constantly on two legs, and when erect was quite equal in height to the average human male. Of the various differences which separate it from the highest apes and the lowest men, it may be said that they bring it closer to the latter than to the former.

One of the bearings of this discovery is upon the original birth-place of the human race. The author believes that the steps in the immediate genealogy of our species were these: *Prothyllobates*: *Anthropopithecus Sivalensis*: *Pithecanthropus erectus*: and *Homo sapiens*. This series takes us to the Indian faunal province and to the southern aspects of the great Himalayan chain, as the region somewhere in which our specific division of the great organic chain first came into being.

THE ANALOGIES OF RELIGIOUS SYMBOLISM.

A LEARNED Hungarian lady, Madame Sofie von Torma, has lately published an interesting little work, a prologue to a large one, in which she points out a number of close analogies or even identities between the symbols and myths of primitive peoples. Its title '*Ethnographische Analogieen; ein Beitrag zur Gestaltungs und Entwicklungsgeschichte der Religionen*' (Jena, 1894).

Beginning with the study of local archaeology, she soon found that the analysis of her home relics took her back to ancient Arcadian and Egyptian prototypes, and the question arose, In what way were they related? To this it is her intention to devote an extended research; and in the volume before us, she states with force and brevity the many remarkable similarities she has noted, and presents the inquiries to which they give rise. The text is accompanied with 127 illustrations.

ETHNIC AFFILIATIONS OF THE JAPANESE.

AFTER a great deal of rambling discussion as to the ethnic relationship of the Japanese, it is gratifying to find a writer who has touched bottom at last, and brings a satisfactory theory with plenty of good evidence to support it. The writer is Dr. Heinrich Winkler, who, in his little pamphlet, *Japaner und Altaier* (Berlin, 1894), offers a solution of the problem which is certainly bound to stand.

He has studied the Japanese both from the anthropometric and the linguistic side. He points out that they present many and positive physical differences from the Chinese type, and can not be classed as a Sinitic people. On the other hand, the measurements bring them into close parallelism with the northern Ural-Altaic peoples, to that group which includes the Samoyeds, the Finns, the Magyars and, in a less degree, the Tungoose. This affiliation is strikingly supported by a careful comparison of languages. There is not a marked morphological trait of the Japanese tongue which is not also found in this Sibiric group. Dr. Winkler rehearses them with brevity and force. What is more, in the opinion of some, the material portion of the language, its vocabulary and radicals, present so many identities with this Ural-Altaic group that their primitive oneness must be conceded.

This, however, is not to be understood as if the Japanese was the Altaic *Ursprache*; but only as one of the children of a common mother, each of which has pursued independent lines of development, though always retaining the family characteristics.

D. G. BRINTON.

UNIVERSITY OF PENNSYLVANIA

HYGIENE.

THE NEW SERUM TREATMENT FOR DIPHTHERIA.

By cultivating the specific bacillus of diphtheria in broth, there is developed in the liquid a peculiar product, which is known as the toxine of this bacillus. When an extensive growth of the bacillus has occurred, so that a considerable quantity of this toxine is developed, the fluid is filtered through a porcelain filter, which permits the soluble toxine to pass through, but retains the bacilli.

If this filtered fluid is sufficiently strong, $\frac{1}{10}$ of a cubic centimeter of it will kill a

guinea pig weighing 500 grammes, in from 48 to 60 hours. The effect produced is in proportion to the quantity injected, just as for any chemical poison, differing in this respect from the action of a fluid containing the bacilli themselves, which might multiply in the body. The bacilli in the fluid might be killed by heating, but this would also decompose the toxine; hence the separation is effected by simple filtration, or by the addition of some substance like tricresol which will kill the bacilli without affecting the toxine.

If small quantities of this toxine be injected under the skin of an animal, commencing with a dose which is not fatal and gradually increasing it, the animal gradually becomes immune to the effects of the poison and after several successive injections can receive a very strong dose without injury. The blood serum of an animal thus rendered immune against diphtheria has the power to confer a similar immunity on other animals if given in sufficient quantity in one dose, thus doing away with the need for the repeated and carefully graduated injections required to produce immunity in the first animal.

To obtain such an anti-diphtheritic serum to be used on man, a horse is injected with the solution of toxine, commencing with from 2 to 5 cubic centimeters and increasing the dose at intervals until within three months as much as 250 cubic centimeters may be injected without producing any serious effect. The horse is more resistant than many other animals to the action of the diphtheritic poison, being naturally somewhat immune. The blood serum of the horse produces no harmful effects on man, if injected in small doses, and it can readily be obtained in considerable quantities without killing the animal.

This serum, taken from a horse which has thus been rendered immune, will not only produce a temporary immunity in man

against the diphtheritic poison, but will antagonize the effects of the diphtheritic poison after this has been already introduced into the system, in other words, it may be employed as a curative agent in cases of diphtheria. The immunity which it produces is a temporary one only, lasting from ten days to three weeks. Its curative effect in cases of the disease depends, to a considerable extent, upon its use in the early stages before the system has been saturated with the poison.

We have not yet sufficient data to speak positively of the value of this anti-diphtheritic serum as a means of treatment of the disease as compared with certain other methods of treatment, especially in the early stages, but the evidence thus far collected seems to indicate that such serum obtained in the proper manner, and used with proper precautions in the hands of experts, is a valuable addition to our means of combatting this terrible malady. The serum can only be properly prepared and tested by a skilled bacteriologist. It must be sufficiently strong in its immunizing power, and at the same time must contain no living pathogenetic germs of any kind. It must also have been comparatively recently obtained from the living animal, for it gradually loses its specific anti-diphtheritic powers. Special antiseptic precautions are also necessary in injecting the serum under the skin in the human subject to prevent the entrance of noxious germs.

One of the most useful points in applying the anti-diphtheritic serum to practical use is to have the cases diagnosed at the earliest possible date, and this can only be done by a skilled bacteriologist. In New York, Boston, and some other cities, means are now provided by which practicing physicians can have such diagnoses promptly made; and if the case of diphtheria can be seen by a physician in its earlier stages, it is possible

to treat it with great hope of success by means of local applications to the throat of certain substances which will quickly destroy the bacillus, and prevent the further production of its peculiar toxine; for example, a solution of tri-cresol of the strength of one per cent. will usually effect this without producing undue irritation or causing any injury to the patient. Those who advocate the use of the immunizing serum say little about the local treatment, but this last is if anything the more important of the two, for the serum does not kill the bacilli which are on the surface of the mucous membrane of the throat, and therefore does not prevent a person rendered immune by it from being the means of spreading contagion.

OYSTERS AS A MEANS OF TRANSMITTING TYPHOID FEVER.

THE *Medical Record* of December 15, 1894, contains a paper by Professor H. W. Conn upon an outbreak of typhoid at Wesleyan University in October and November last, which included about twenty-six cases. When the serious character of the outbreak was recognized, an investigation as to causes was begun. The water supply was tested, and the house plumbing was examined without result. It was found that the disease was almost entirely limited to the members of three fraternities. The period of incubation of typhoid—that is, the time which elapses between the taking of typhoid bacillus into the body and the definite manifestation of the disease—is usually from ten to fourteen days, but may range from seven to twenty-eight days. The first cases of the fever among the students appeared October 20th, and suspicion soon fell upon the fraternity suppers of October 12th. Careful examination of the food supplied at these suppers showed that raw oysters, obtained by each of the three fraternities from the same oyster dealer, were the only things

which were peculiar to their suppers, and inquiry was at once directed to these oysters. It was found that they had been obtained from the deep water of Long Island Sound and had been deposited in the mouth of a fresh water creek to freshen, or to 'fatten,' as it is termed, since under such circumstances the oyster absorbs the fresh water by osmosis and therefore swells and becomes plump. Further inquiry showed that, within about three hundred feet of the place where the oysters had been deposited, was the outlet of a private sewer coming from a house in which were two cases of typhoid fever at the time when the oysters were taken up and sent to the University.

The typhoid bacillus will live for a time in salt or brackish water, and it was proved by trial that if such bacilli are forced in between the two valves of the shell they remained alive long enough to enable the oysters to be carried and used at the fraternity suppers. Whether the bacillus will grow and multiply in living or dead oysters has not yet been determined, but experiments on this point are in progress.

It will be seen that the evidence that the outbreak of typhoid was produced by these oysters is purely circumstantial, but the links in the chain are well connected and strong.

It is by no means certain that there were any typhoid germs within the oysters or the oyster shells when they were sent to Middletown. If the shells were smeared on the outside with typhoid excreta some particles of this might easily have gotten among the oysters during the process of opening them. But it is evident that oysters grown or fattened in positions where sewage may come in contact with them are dangerous if eaten raw.

THE EVOLUTION OF INVENTION.

IN a recent study that I have made on the evolution of invention I have divided

the changings which underlie all examples of the process into those—

1. Of the thing or process, commonly called inventions.
2. Of the apparatus and methods used.
3. Of the rewards to the inventor.
4. Of the intellectual activities involved.
5. Of society.

Each one of these has undergone an evolution or elaboration, from monorganism to polyorganism, from simplicity to complexity, from individualism to coöperation, from use to comfort, and so on. This statement needs no extended proof; the roller mill is the descendant of the metals, machinery springs from tools, the device beneficial only to its originator becomes the world-embracing and world-blessing invention; the happy thought of one person at last comes to be the beneficent result of an endowed and perennial coöperation, a perpetual repository of invention renewed constantly by the removal of the senescent and the introduction of new and trained minds as in a university.

Now it requires great patience to get together the material evidence of this unfolding or evolution. The mental processes are no longer in sight. The nearest approach to them are the makeshifts of savages, and their minds are almost a sealed book. It has therefore occurred to the writer that among the questions proposed to those who are collating information relating to the psychic growth of children there should be a short series respecting the unfolding of the inventive faculty or process, the finding out originally how to overcome new difficulties or surmounting old ones in new ways.

O. T. MASON.

SCIENTIFIC LITERATURE.

Popular Lectures and Addresses.—Vol. II., *Geology and General Physics.*—LORD KELVIN.—Macmillan & Co., New York and London. Pp. 599. Price \$2.00.

It is characteristic of the work of a really great genius, either in Science, Literature or Art, that it is not displaced and cannot be displaced by that which may come after it.

A bit of scientific work may later be found to be erroneous as to data, and, therefore, in the wrong as to conclusions, but if it be the work of an aggressive, original thinker, it will always have great value. In the brilliant galaxy of physicists, or, as he would himself call them, natural philosophers, which the present century has produced, it is moderation to say that none outshines Lord Kelvin, and it will not be denied that none has equalled him in aggressiveness and originality. The range of subjects upon which he has touched during his long and active life is so extensive as to certainly justify the use of the term Natural Philosopher in its broader sense (and capitalized at that), for he has never touched a department of human knowledge without leaving it richer and more extensive for his contact with it. That he has not been invariably infallible is recognized by no one more fully than by himself, and the new editions of his earlier papers which have been issuing from the press at intervals during the past few years, bear most interesting evidence of his readiness to change his attitude on great questions whenever the verdict of later investigations is against him. It is delightful to note the occasional parenthetical '*not*' put to-day into a sentence which twenty years ago declared very positively that '*there is*' so and so, or, '*we can,*' etc., completely reversing the meaning of statements which were once made with a good degree of confidence. Whatever else may be said, it cannot be asserted that Lord Kelvin has ever lacked the courage to express his own views in most forcible and unmistakable language. Indeed, in this respect, especially, he has set a splendid standard of unswerving scientific honesty

for the innumerable workers who have been, and will be, more or less influenced by his methods and their tremendous productiveness.

His views as to the proper attitude of the philosopher in his relations to unexplored regions of human experience are concisely expressed in this noble sentence from his Presidential Address before the British Association for the Advancement of Science, in 1871: "Science is bound by the everlasting law of honor to face fearlessly every problem which can fairly be presented to it." When he comes, however, to touch upon some problems which have long been of great interest to the human race, but which have been assumed, usually, to lie outside the domain of experimental or exact science (and he touches upon them not infrequently in the volume under consideration), it is not difficult to see a very decided bias towards certain views, and a promptness to accept propositions not always well supported by evidence, very greatly in contrast with what is found in more vigorously scientific discussion.

This series of popular lectures and addresses is published in three volumes, the first and third having already appeared. The second (issued later than the third), to which attention is now invited, contains the important addresses on geological physics which have attracted so much attention during the past quarter of a century, together with a number of lectures and short papers on subjects related to general physics and extracts from addresses as president of the Royal Society since 1890. The geological papers are of great interest and have had much to do with the moulding of the views of geologists as to Dynamical Geology. The series begins with a short note covering but a single octavo page, entitled, '*The Doctrine of Uniformity in Geology Briefly Refuted,*' read at Edinburgh in 1865. It fairly 'opens the ball,' and may be regard-

ed as the key note to the more elaborate disquisitions which followed at intervals up to recent dates. These papers are so well known, or ought to be so well known, to all geologists as to make it only necessary to say here that they will be found collected in this volume in convenient form and with a few notes and occasional comments by the distinguished author, made while the collection was being prepared for the press. The most important of the earlier papers are the address '*On Geological Time*,' given in Glasgow, early in 1868, and that on '*Geological Dynamics*' at the same place about a year later. In the first of these will be found the somewhat severe strictures upon '*British Popular Geology*' which brought forth the interesting and pointed criticisms of Huxley in his address to the Geological Society of London, and in the second the replies to Huxley's criticisms and further remarks upon the subject. Nearly ten years later came a '*Review of the Evidence Regarding the Physical Condition of the Earth*,' read at the British Association meeting at Glasgow; two papers read before the Geological Society of Glasgow, on '*Geological Climate*,' and on the '*Internal Condition of the Earth*;' and after the lapse of another ten years a paper before the same society on '*Polar Ice Caps and their Influence in Changing Sea Levels*.' In these much of the ground of the earlier addresses is again gone over, in the light of later discovery in geology, physics and astronomy.

Indeed these same topics recur again and again, sometimes incidentally in other addresses in the volume, and Lord Kelvin makes it entirely clear that in thus taking up the discussion of geological problems and applying to them the methods and data of physics and astronomy, he does not wish to be considered an interloper. In his reply to Huxley, who had rather pointedly intimated that view of the situation, he good-naturedly remarks: "For myself I am anxious to be regarded by geologists,

not as a mere passer-by, but as one constantly interested in their grand subject, and anxious in any way, however slight, to assist them in their search for truth."

It seems difficult to over-estimate the importance of these geological addresses, not only to the geologist, but to the physicist as well. They not only have a general interest to both, but are of special importance to each. To the one they open new possibilities of a somewhat exact and satisfactory treatment of a most important but hitherto rather unmanageable department of his subject; and to the other they offer a most instructive illustration of the power and scope of the methods of exact science, when applied by one who may justly be called not a master, but *the* master.

Of the other addresses, none, of course, is more important or interesting than the British Association Presidential Address of 1871, so well known to all. One of the earliest, on '*The Rate of a Clock or Chronometer as Influenced by the Mode of Suspension*,' is most entertaining and suggestive as an example of the many 'side-lights' of a remarkable intellectual activity. Of great historical value is the Royal Institution lecture of 1856 on the '*Origin and Transformation of Motive Power*'—already republished in Volume II. of the '*Mathematical and Physical Papers*;' and one of the most interesting is that of late date (1892) on the '*Dissipation of Energy*.' In this much attention is given to the principle of Carnot, and here also occurs a remarkable statement which the author himself has thought worth while to print in italics;—it is:—"The fortuitous concurrence of atoms is the sole foundation in Philosophy on which can be founded the doctrine that it is impossible to derive mechanical effect from heat otherwise than by taking heat from a body at a higher temperature, converting at most a definite proportion of it into mechanical effect, and giving out the whole residue to matter at a lower temperature."

The address on the opening of the Bangor Laboratories will be of interest to all who have to do with their like; that on the occasion of the unveiling of Joule's statue will interest everybody who cares for or who knows of the greatest generalization of modern science. In short, every page of this volume is deserving of the careful perusal of all who are devoted to Natural Philosophy in its most comprehensive sense, and who wish to know something of the spirit of one whose splendid contributions to physical science are, as a whole, greater than those of any other philosopher of the present time.

The mechanical execution of the book does not seem to be quite in keeping with the classical character of its contents, and its pages are occasionally marred by negligent proof reading. T. C. MENDENHALL.

WORCESTER POLYTECHNIC INSTITUTE.

Laws of Temperature Control of the Geographic Distribution of Life.

In the December issue of the *National Geographic Magazine*, Dr. C. Hart Merriam announces the discovery of the laws of temperature control of the geographic distribution of terrestrial animals and plants. Dr. Merriam has been engaged on this problem for sixteen years and believes he has at last obtained a formula which fulfills the requirements. He states that in the Northern Hemisphere animals and plants are distributed in circumpolar belts, the boundaries of which follow lines of equal temperature rather than parallels of latitude. Between the pole and the equator there are three primary belts or regions—Boreal, Austral and Tropical. In the United States the Boreal and Austral have each been split into three secondary trans-continental zones, of which the Boreal are known as the Arctic, Hudsonian and Canadian; and the Austral as the Transition, Upper Austral and Lower Austral.

The temperature data computed and plotted on maps as isotherms are not available in locating the boundaries of the zones, because they show the temperature of arbitrary periods—periods that have reference to a particular time of year rather than a particular degree or quantity of heat.

It is assumed that the distribution of animals and plants is governed by the temperature of the season of growth and reproductive activity—not by that of the entire year. The difficulty is to measure the temperature concerned.

Physiological botanists have long maintained that "the various events in the life of plants, as leafing, flowering and maturing of fruit, take place when the plant has been exposed to a definite quantity of heat, which quantity is the sum total of the daily temperatures above a minimum assumed to be necessary for functional activity." The minimum used by early botanists was the freezing point (0° C or 32° F), but recent writers believe that 6° C or 42.8° F more correctly expresses the temperature of the awakening of plant life in spring. "The substance of the theory is that *the same stage of vegetation is attained in any year when the sum of the mean daily temperatures reaches the same value*, which value or total is essentially the same for the same plant in all localities. This implies that the period necessary for the accomplishment of a definite physiological act, blossoming, for instance, may be short or long, according to local climatic peculiarities, but the total quantity of heat must be the same. The total amount of heat necessary to advance a plant to a given stage came to be known as the *physiological constant* of that stage." But students of geographic distribution are not concerned with the physiological constant of any stage or period in the life of an organism, but with *the physiological constant of the species itself*—if such a term may be used. "If it is true that the same stage of vegetation is

attained in different years when the sum of the mean daily temperatures reaches the same value, it is obvious that the physiological constant of a species must be *the total quantity of heat or sum of positive temperatures required by that species to complete its cycle of development and reproduction.*" Now, "if the computation can be transferred from the species to the zone it inhabits—if a *zone constant* can be substituted for a *species constant*—the problem will be well nigh solved." This Dr. Merriam has attempted to do. "In conformity with the usage of botanists, a minimum temperature of 6°C (43°F) has been assumed as marking the inception of the period of physiological activity in plants and of reproductive activity in animals. The effective temperatures or degrees of normal mean daily heat in excess of this minimum have been added together for each station, beginning when the normal mean daily temperature rises higher than 6°C in spring and continuing until it falls to the same point at the end of the season." The sums thus obtained were plotted on a large scale map of the United States, and isotherms were run which were found to conform to the *northern* boundaries of the several zones. This is shown by colored maps. The data seem to justify the statement that "*animals and plants are restricted in northward distribution by the total quantity of heat during the season of growth and reproductive activity.*"

In the case of the *southern* boundaries of the zones, it was assumed that animals and plants in ranging southward would encounter, sooner or later, a degree of mean summer heat they are unable to endure. "The difficulty is in ascertaining the length of the period whose mean temperature acts as a barrier. It must be short enough to be included within the hottest part of the summer in high northern latitudes, and would naturally increase in length from the north southward. For experimental pur-

poses, and without attempting unnecessary refinement, the mean normal temperature of the six hottest consecutive weeks of summer was arbitrarily chosen and plotted on a large contour map of the United States, as in the case of the total quantity of heat."

On comparing this map with the zone map, the isotherms of 18°, 22° and 26°C were found to conform respectively to the southern boundaries of the Boreal, Transition and Upper Austral zones, leading to the belief that "*animals and plants are restricted in southward distribution by the mean temperature of a brief period covering the hottest part of the year.*"

Except in a few localities the northern boundary of Austral species coincides with the southern boundary of Boreal species, but for a distance of more than a thousand miles along the Pacific coast a curious overlapping and intermingling of northern and southern types occurs. On looking at the temperature maps this is at once explained, for the mean temperature of the six hottest consecutive weeks from about lat. 35° northward to Puget Sound is truly Boreal, being as low as the mean of the corresponding period in northern Maine and other points well within the Boreal zone. On the other hand, the total quantity of heat is found to be the same as that required by Austral species. "It is evident, therefore, that the principal climatic factors that permit Boreal and Austral types to live together along the Pacific coast are a low summer temperature combined with a high sum total of heat."

A table is given showing the actual governing temperatures, so far as known, of the northern and southern boundaries of the several zones.

In conclusion, Dr. Merriam calls attention to the subordinate value of humidity as compared with temperature. "Humidity and other secondary causes determine the

presence or absence of particular species in particular localities within their appropriate zones, but temperature predetermines the possibilities of distribution; it fixes the limits beyond which species cannot pass; it defines broad transcontinental belts within which certain forms may thrive if other conditions permit, but outside of which they cannot exist, be the other conditions never so favorable."

Grasses of Tennessee—Part II.—F. LAMSON-Scribner.—University of Tennessee, Agric. Exper. Sta. Bull., VII. 1-141, 187 figures. 1894.

The first part of this important work treating of the structure of grasses in general, issued two years ago, is now supplemented by the part here noticed, containing descriptions and figures of all species known by the author to inhabit Tennessee. Carefully prepared keys to the genera and species are a feature of the book. The cuts are good, although printed on paper hardly firm enough to bring them out to the best advantage. The descriptions are diagnostic and couched in strictly technical language; on this point it is remarked: "Attempts to avoid technical or 'hard' words often result in obscuring the meaning of the author, and an undue simplicity of expression is often apt to be offensive by implying a lack of intelligence on the part of the reader." As the book is intended primarily for the farmers of the State, this may be considered by some as a position of doubtful value.

It is to be regretted that the rules of nomenclature adopted by the botanists of the American Association for the Advancement of Science, which are practically those approved by the zoölogists, have not been strictly followed. This will seriously hamper the usefulness of the book, for some of the names used by Prof. Scribner have become obsolete.

N. L. B.

NOTES.

PHYSICS.

THE newly discovered gas is to be the subject of a discussion at a meeting of the Royal Society on January 31st, when Lord Rayleigh and Prof. Ramsay will present their paper. This will be the first meeting under a resolution of the Council of the Society passed last session, whereby certain meetings, not more than four in number, are to be devoted every year each to the hearing and consideration of some one important communication, or to the discussion of some important topic.—*Nature*.

PERSONAL.

THE University of Berlin is seriously crippled by the deaths of Helmholtz and Kundt. Their places cannot be filled, but Prof. Kohlrausch will probably be called to one of the vacant chairs.

THE *Physical Review* has published excellent portraits of Helmholtz, Kundt and Hertz, with biographical sketches by the editor-in-chief, Professor Nichols. Probably the best account so far published in English of the work of Helmholtz is that contributed to the *Psychological Review* for January by Professor Stumpf, of the University of Berlin.

MR. F. Y. Powell, of Christ's College, succeeds Froude in the Regius Professorship of Modern History at Oxford.

ZOÖLOGY.

A PICTURE-PUZZLE of a remarkable kind appears in the *Zoölogist* for December. It is a reproduction of two photographs of a Little Bittern, showing the strange attitude assumed by the bird to favor its concealment. One of the figures shows the bird standing in a reed-bed, erect, with neck stretched out and beak pointing upwards; and in this position it is difficult to distinguish the bird at all from the

reeds. The eye is deceived in a similar manner when the bird is crouching against a tree-stump at the river side. Mr. J. E. Harting thinks that the curious attitudes adopted by the bird, on finding itself observed, are assumed in the exercise of the instinct of self-preservation. He mentions a similar habit, observed and described by Mr. W. H. Hudson, in the case of South American Little Heron, which frequents the borders of the La Plata, and is occasionally found in the reed-beds scattered over the pampas. Without the aid of dogs it was found impossible to secure any specimens of this bird, even after making the spot where one had alighted.—*Nature*.

NEW PUBLICATIONS.

Astronomy and Astro-Physics will hereafter be called the *Astrophysical Journal* and will be published from the University of Chicago, under the editorship of Profs. Payne and Keeler and a board of the leading men of science in this department.

A monthly *Magazine of Travel*, somewhat practical and popular in character, will hereafter be published from 10 Astor Place, New York.

The Aeronautical Annual for 1895, soon to be published by W. B. Clarke & Co., Boston, will contain reprints of some early treatises on aeronautics, among them da Vinci's *Treatise on the Flight of Birds*, Sir George Gayley's *Aerial Navigation* (1809), *A Treatise upon the Art of Flying*, by Thomas Walker (1810), and Franklin's aeronautical correspondence.—*Critic*.

P. Blakiston, Son & Co. announce *The Dynamics of Life*, by William R. Gowers, M. D., of London.

SOCIETIES AND ACADEMIES.

THE TEXAS ACADEMY OF SCIENCE.

DECEMBER 31, 1894.

DR. HALSTED, President, in the chair.

JAMES E. THOMPSON; *Address*.

DAVID CERNA; *The phonetic arithmetic of the ancient Mexicans*.

WILLIAM KEILLER; *Descriptive anatomy of the heart*.

THOMAS FLAVIN; *Developmental anatomy and pathology of the kidneys*.

THOMAS U. TAYLOR; *Present need of engineering education in the South*.

ROBERT A. THOMPSON; *The storm-water storage system of irrigation*.

T. H. BRYANT, *Acting Secretary*.

NEW BOOKS.

Progress in Flying Machines. O. CHANUTE. New York, The American Engineer and Railroad Journal. 1894. Pp. iv+308.

Lectures on the Darwinian Theory. A. M. MARSHALL. Edited by C. F. MARSHALL. London, D. Nutt; New York, Macmillan & Co. 1894. Pp. xx+236. \$2.25.

Sea and Land. Features of Coasts and Oceans with Special Reference to the Life of Man. N. S. SHALER. New York, Charles Scribner's Sons. 1894. \$2.50.

Text-book of Invertebrate Morphology. J. F. McMURRICH. New York, Henry Holt & Co. 1894. Pp. 294. \$4.00.

The Planet Earth. An Astronomical Introduction to Geography. RICHARD A. GREGORY. London and New York, Macmillan & Co. 1894. Pp. viii+105. 60c.

Physiology for Beginners. M. FOSTER and LEWIS E. SHORE. New York and London, Macmillan & Co. 1894. Pp. ix+241. 75c.

The Rise and Development of Organic Chemistry. CARL SCHORLEMMER. Revised edition, edited by ARTHUR SMITHELLS. London and New York, Macmillan & Co. 1894. Pp. ix+280.

Woman's Share in Primitive Culture. O. T. MASON. New York, D. Appleton & Co. 1894. Pp. xiii+295.